

# Philosophy of the Social Sciences

<http://pos.sagepub.com/>

---

## Why Science Studies Has Never Been Critical of Science : Some Recent Lessons on How to Be a Helpful Nuisance and a Harmless Radical

Steve Fuller

*Philosophy of the Social Sciences* 2000 30: 5

DOI: 10.1177/004839310003000101

The online version of this article can be found at:

<http://pos.sagepub.com/content/30/1/5>

---

Published by:



<http://www.sagepublications.com>

**Additional services and information for *Philosophy of the Social Sciences* can be found at:**

**Email Alerts:** <http://pos.sagepub.com/cgi/alerts>

**Subscriptions:** <http://pos.sagepub.com/subscriptions>

**Reprints:** <http://www.sagepub.com/journalsReprints.nav>

**Permissions:** <http://www.sagepub.com/journalsPermissions.nav>

**Citations:** <http://pos.sagepub.com/content/30/1/5.refs.html>

>> [Version of Record](#) - Mar 1, 2000

[What is This?](#)

# Why Science Studies Has Never Been Critical of Science

## Some Recent Lessons on How to Be a Helpful Nuisance and a Harmless Radical

STEVE FULLER  
*University of Warwick*

Research in Science and Technology Studies (STS) tends to presume that intellectual and political radicalism go hand in hand. One would therefore expect that the most intellectually radical movement in the field relates critically to its social conditions. However, this is not the case, as demonstrated by the trajectory of the Parisian School of STS spearheaded by Michel Callon and Bruno Latour. Their position, “actor-network theory,” turns out to be little more than a strategic adaptation to the democratization of expertise and the decline of the strong nation-state in France over the past 25 years. This article provides a pre-history of this client-driven, contract-based research culture in U.S. sociology of the 1960s, followed by specific features of French philosophical and political culture that have bred the distinctive tenets of actor-network theory. Insofar as actor-network theory has become the main paradigm for contemporary STS research, it reflects a field that dodges normative commitments in order to maintain a user-friendly presence.

There are many ironic features about the development of the interdisciplinary field of Science and Technology Studies (STS). Perhaps most ironic of all is the field’s handling of its own history. STS treats its past unproblematically, always tracing its proper origins to the Edinburgh School after having paid tribute to Thomas Kuhn as the mythical progenitor. (This is true even of such well-informed “second generation” texts as Golinski 1998.) Indeed, in classic Kuhnian fashion, STS is prone to conceptualize its own history as an evolving response to theoretical and methodological issues surrounding the nature of science that transcend national boundaries. Yet, even a cursory understanding of the different post-World War II science policies pursued in the founding nations of STS—the United Kingdom, the

---

*Received* 12 July 1999

*Philosophy of the Social Sciences*, Vol. 30 No. 1, March 2000 5-32  
© 2000 Sage Publications, Inc.

United States, and France—should be enough to cast doubts on any straightforward history of the field. In a new book on Kuhn's significance for contemporary understandings of science and society (Fuller 2000), I consider the distinctive American and British contexts from which STS arose. However, in what follows, I focus the social epistemologist's gaze on the most self-consciously "progressive" wing of the field, which emanates from L'Ecole des Mines in Paris and whose most famous proponent is Bruno Latour.

I begin by presenting the Parisian position—"actor-network theory"—as a culmination of the founding myth of STS. I explain its current popularity in terms of the client-driven environment of its conception, which is increasingly becoming the norm in academic work. Precedents for this perspective can be found in American sociology in the 1960s, especially in a critical exchange between Howard Becker and Alvin Gouldner. The political and epistemological background to the Parisian position forms the bulk of this article. I portray actor-network theory as a classic beneficiary of other people's miseries, in this case, a democratized and status-degraded French engineering profession. Gaston Bachelard turns out to be a significant epistemological figure because of his explicit portrayal of scientists as akin to a philosophically exploited proletariat, a view that suits French engineers after 1968. Finally, I show the pervasive effects of this mentality on STS, most notably an aversion to normative judgments and even an open antagonism to the adoption of "critical" perspectives. I conclude that this tendency, while understandable in terms of the client-driven nature of much STS research, throws doubt on the intellectual integrity of the field, something which social epistemologists urgently need to address.

#### DEMISTIFYING THE FOUNDING MYTH OF SCIENCE AND TECHNOLOGY STUDIES

According to the founding myth of STS, we may think of philosophers and sociologists of science as the two sides of a dialectic. Both are fixated with the two main public symbols of science—the special laboratory site where knowledge is produced and the special talk of truth, objectivity, and reason that goes on around that site. Both suppose that the secret of science lies in revealing the relationship between the two symbols. Whereas the philosophers invest powers of "referentiality" in the talk that enables science to transcend the site,

the sociologists impute “indeterminacy” to such talk to explain how it obscures access to that site. To be sure, the loose and often reconstructed (perhaps even fabricated) character of what we say in relation to what we do is a common feature of everyday life. Consequently, to draw attention to it under the rubric of “indeterminacy” serves a critical and polemical function only for those who would otherwise think that the language of science is somehow exempt by virtue of its unique transparency and rigor.

While many philosophers—and the scientists who take them seriously—have held such views in the past, with the decline of philosophical commitment to “positivism” or “realism” (two quite different positions that agree on the point at issue), it is no longer clear how much more mileage STS can gain by highlighting the “underdetermined” character of scientific discourse, aside from the sheer accumulation of case studies that confirm this virtually conceded point of referential indeterminacy. In that respect, even accepting STS’s founding myth on its own terms, the field remains stuck in the second moment of its dialectic with the philosophy of science. The argument from there simply concerns the step that logically follows this collectively assumed history. Thus, the most wide-ranging and seemingly radical professional debate within STS in recent years, the so-called epistemological chicken controversy, is ultimately about nothing more than alternative extrapolations from a common past. (For the original works, see Collins and Yearley 1992; Latour and Callon 1992; see also Fuller 1996.) The rhetoric of the debate is most naturally explained as niche differentiation in what has become a crowded field for STS research.

On the one hand, we have Harry Collins and Steven Yearley, who propose to stick to the letter of the field’s Edinburgh origins by extending the signature Sociology of Scientific Knowledge (SSK) methods of ethnography, discourse analysis, and critical historical scholarship to more domains of inquiry—beyond the usual academic research sites to environmental movements and knowledge engineering firms—without deepening or challenging SSK’s fundamental assumptions. In their hands, STS would remain autonomous not only from the folk theories that scientists and technologists use to explain their activities but also from the more sophisticated theories that social scientists have developed to explain other aspects of social life.

On the other hand, we have the self-avowed radicals of the debate, the Paris School of STS represented by Michel Callon and Bruno Latour. They believe that adhering to the original Edinburgh spirit

requires breaking with its letter; hence, they call for a generalized application of SSK's symmetry principle. The complexity of technoscientific networks revealed in their studies cannot be accounted for simply by invoking social factors, however "symmetrically" they are applied to successful and failed courses of action. *Natural* factors need to be invoked as well—and just as symmetrically. Not surprisingly, scientists (e.g., Labinger 1995) who follow the STS literature have welcomed the Parisian turn, since it clearly reopens the door to traditional, even commonsensical, explanations of science that incorporate both social and natural factors "interacting" to produce, say, an experimental outcome. It would seem, then, that we have reached one of those all too familiar Molierean moments in academic life when a move that appears radical within the terms of a paradigm is equivalent to the prose that everyone else outside the paradigm has been always speaking (albeit now with a French accent).

The impasse between Collins and Latour is symbolized by the Janus-faced character of STS's much vaunted case study methodology. On the one hand, in Collins's view, case studies create intellectual entitlements for the STS practitioner that effectively restrict the "community of inquirers" simply to those with similar training and experience. On the other hand, in Latour's view, because case studies are typically evaluated merely in terms of their descriptive adequacy ("Does it tell a good story?"), and not some larger normative context, they can be of potential use to a wide range of users, most notably those who do *not* share the STS researcher's personal or professional commitments. But regardless of whether Collins's or Latour's view prevails, the dynamic spirit of critical inquiry loses.

On the surface, Collins and Latour appear to be arguing about the future of a specialized field of inquiry called "Science and Technology Studies," but in fact their attitudes reflect a fundamental disagreement about the prospects of their own knowledge production site, the *university*. Collins has steered clear of collaborating with the state and industry, whereas Latour has been housed in an institution that has had to develop such networks in order to sustain its research programs. There is nothing especially mysterious about this difference. Their respective national academic contexts largely explain it (Fuller 2000, chap. 7). But the difference also reflects an emerging schism, namely between what fashionable science policy theorists call "Mode 1" and "Mode 2" conceptions of knowledge production. (For the provenance of this jargon, see Gibbons et al. 1994.) Collins represents the Mode 1 conception of university-protected, paradigm-

driven inquiry, whereas Latour represents the Mode 2 conception of policy-driven “postdisciplinary” research, which welcomes the university’s permeability to extramural concerns.

If the Paris School finds itself in the vanguard of STS research, it is because the material conditions under which its work has been done—Mode 2 knowledge production—has come to be the norm throughout an academy increasingly moved by market pressures. There is much at stake, once the epistemological chicken dispute is amplified in these terms. What does it mean to be “political” or, for that matter, “public” in this volatile context of inquiry? How are the universal aspirations of inquiry to be reconciled with the pressure to be both professional and client driven? What fate awaits the flickering spirit of criticism in an intellectual world increasingly beholden to the idea that there is safety in numbers?

To be sure, this is not the first time these questions have been posed. They can be traced to American sociology’s soul-searching in the 1960s, when a culture of academic “radicalism” was first operationalized as a case study methodology that delivered on a platter those on the social periphery to more centrally located clients. The watershed exchange from this period was Alvin Gouldner’s (1968) stinging indictment of “the theory and practice of cool” that he accused Howard Becker (1967) of promoting under the banner of symbolic interactionism and labeling theory in his 1966 presidential address to the Society for the Study of Social Problems. Not surprisingly, Becker’s address has been presented as a mainstream sociological precursor of the STS research perspective (Star 1995, p. 1). Before turning to the fully articulated Parisian version of such opportunistic radicalism, a look at the Gouldner-Becker debate is in order.

In answer to his own question, “Whose side are we on?” Becker seemed to say society’s underdogs, those psychosocial deviants whose very existence constituted “social problems” for those who funded much sociological research in the era of the welfare state: the insane, the poor, the addicted, the criminally inclined, and the variously strange. On the surface, Becker’s argument called for the articulation of these deviant perspectives, giving them a clarity and coherence that “the establishment” would otherwise deny them. But Gouldner questioned Becker’s sincerity. The accounts promoted by Becker’s “cool” approach to social problems cast the deviants as living in self-contained “worlds” where deprivation effectively becomes a mark of positive identity. But exactly who benefits from such a portrayal? Here is Gouldner’s bracing answer:

The new underdog sociology propounded by Becker is, then, a standpoint that possesses a remarkably convenient combination of properties: it enables the sociologist to befriend the very small underdogs in local settings, to reject the standpoint of the "middle dog" respectables and notables who manage local caretaking establishments, while, at the same time, to make and remain friends with the really top dogs in Washington agencies or New York foundations. While Becker adopts a posture as the intrepid preacher of a new underdog sociology, he has really given birth to something rather different: to the first version of new Establishment sociology. . . . It is a sociology that succeeds in solving the oldest problem in personal politics: how to maintain one's integrity without sacrificing one's career, or how to remain liberal although well-heeled. (Gouldner 1968, 111)

In other words, Becker's epistemologically liberating relativism made it easier for a distant power—the inquirer's client—to contain the natives by redressing the balance of power enjoyed by a more immediate (and competing) oppressor of the natives. In this respect, Becker was following a path laid down by the social anthropologist Edward Evans-Pritchard, who urged his Oxford students to learn native African languages in the 1930s, not least because they would then be well placed to maintain the natives' confidence against Britain's imperial rivals, especially the Germans (Fuller 2000, chap. 2, sec. 8). In both Becker's and Evans-Pritchard's cases, the people who are given voice in the social researcher's narrative are not necessarily the ones best positioned to exploit their newly articulated identities. They can be easily turned into pawns in a power struggle. Indeed, whatever strength the natives derived from their invisibility and marginality is generally lost. If the natives are not then connected with a social movement capable of extending their sphere of action, their interests will have been effectively rendered a target for ventriloquism by a higher authority.

This perverse consequence would appear even more troubling, were the sociologist not its direct beneficiary, much in the manner of what Georg Simmel called the *tertius gaudens*, the third party who benefits from the miseries of others. For, by stopping short of taking any direct responsibility for what her client does with the information gathered on the natives, the sociologist articulates the self-imposed limits of her paradigm, which in turn reinforces (for the benefit of current and future clients) her "professionalism." Moreover, as Gouldner astutely observed, when Becker acknowledged the inevitability of bias and value commitment in his research, he made it seem—as STS

researchers continue to do—that these are matters of personal prediction that would vary across inquirers. But in fact, the structural position of the sociologist in relation to both clients and subjects virtually ensured the reproduction of specific power relations, regardless of the content of the sociologist's findings (Gouldner 1968, 112).

To be sure, the social researcher's *tertius gaudens* role is based at least as much on survival as self-aggrandizement. An apt comparison is with the moral situation of the informant in wartime, except that here we are talking about a marketplace environment in which increasing amounts of academic research is done by contract workers whose employment prospects are determined on a grant-to-grant basis. Under such a regime, if researchers do not provide quality information about their subjects to clients, they will be quickly replaced by someone more willing and able to do so. Thus, the researcher's credibility as a witness is always at issue. Gouldner detected this tendency in Becker's concern that sociologists might succumb to sentimentalizing their subjects and thereby "lose their cool." For Gouldner, the object of such sentimentalization, people's suffering, is morally and politically significant but, for Becker, they prove practically and theoretically inconvenient. The idea that suffering might be integral to people's experience implies that the oppressed might see themselves not in terms of some positive identity (or distinct "social world" they inhabit) but exactly as their oppressors see them—driven primarily by their subjugated status—and that their suffering was either avoidable in the past or eliminable in the future.

For the client-centered sociologist to introduce such a measure of indeterminacy and changeability about the natives into her report would be tantamount to admitting that she had not mastered the means of controlling them. It would also draw undue attention to the client's role in maintaining the power relations revealed in the sociologist's report. Of course, there is a metaphysical remedy for such uncomfortable revelations. It involves flattening the ontology of the social world so that structures are replaced by networks, and all parties are presented as exerting their own kind of power over each other, according to the alliances they can form in a given circumstance. Claims that the natives are subjugated or suffering are thus converted into ones about their hidden competences and agency. As a result, the contingency of the natives' condition may not be reduced, but the client's responsibility for it is. With that in mind, we turn to the Paris School of STS.



CAPTURE OR BE CAPTURED:  
A SOCIAL EPISTEMOLOGY OF THE PARIS SCHOOL OF STS

For the last 10 years, the major source of intellectual excitement in STS has come from France, particularly the Center for the Sociology of Innovation (CSI) at L'Ecole Nationale Supérieure des Mines in Paris, which is one of *les grandes écoles*, the elite national institutions of higher education. The two principal theorists, Michel Callon and Bruno Latour—the one trained in physics and economics and the other in philosophy and anthropology—have developed a version of STS known as “actor-network theory,” which promises nothing less than a complete makeover of the social sciences, specifically by defining “networks” as the stuff out of which both individual identity and social organization are constructed.

The role of networks in social life has long been recognized in the social sciences, but usually as an intermediate level of social organization between, say, a face-to-face group and an institution. However, Callon and Latour redefined these multiple levels as networks of varying lengths, resiliency, and rates of growth. Moreover, the advent of global electronic communications has reinvigorated the appeal to “network” in social theory, though the most ambitious attempt to see the entire contemporary scene in these terms (Castells 1996-98) makes no reference whatsoever to actor-network theory, despite a general familiarity with recent work in the social history of technology (for a critique, see Fuller 1999c).

From a sociology of knowledge standpoint, the most striking feature of actor-network theory's position in STS research is that it has been more popular as a research agenda in the English-speaking world than in France itself, even though the French context for studying the social character of science is rather unique. It has included from the outset a very strong state-led “strategic research” initiative designed to promote what the late socialist leader Francois Mitterand in his first successful bid for the presidency in 1981 called “technoculture.” Here Mitterand had taken a page from Harold Wilson's 1964 rhetorical playbook in using science and technology as rallying points to unite the country around a vision of economic progress (Turney 1984, 221-22). As part of this campaign, STS would be instrumental in tracking the application and reception of technoscientific innovation.

In fleshing out his conception of technoculture, Mitterand contrasted his own *politique de filieres* with the *politique de creneaux* of his predecessor, the neo-Gaullist Valéry Giscard d'Estaing. This switch in

metaphors would become second nature in STS by the end of the 1980s, as the mark of “technoscience” becomes the strength of its networks rather than the achievement of specific goals. As it turns out, Giscard had also promoted science and technology, but in the context of triaging French productive capacities by targeting research funds to industries—especially aerospace, arms, nuclear energy, and transport—where France enjoyed a comparative advantage on the world scene.

However, France’s world share in these areas did not grow during the Giscard presidency (1974-1981), and overall the economy slumped. Mitterand’s remedy was to make information technology integral to *all* sectors of economy, including the old industrial warhorses of steel, mining, and shipbuilding. In the end, France continued to lag on the world economic stage and its industrial sector imploded. Yet, as unprecedented numbers of workers lost their jobs under Mitterand, they could at least take comfort in their high-tech skills which, in principle, enabled them to move into the booming service sector of the economy. Indeed, among major industrialized nations, the distinctiveness of France’s economic profile over the last half-century has been the rapid conversion from agriculture to service as the leading source of wealth and employment (Gildea 1996, 86ff.).

Although the CSI had been founded toward the end of the de Gaulle era in 1967, 1980 had marked the beginning of the collaboration of Callon and Latour in what they first called “the sociology of translation” (e.g., Callon, Law, and Rip 1986; Latour 1987). “Translation” was meant broadly to cover the process whereby one thing represents another so well that the voice of the represented is effectively silenced. Central to this process is the capacity of something to satisfy—and thereby erase—a desire. Callon and Latour exploited the Latin root of “interest” as *interesse* (“to be between”) to capture this capacity, which reverses the ordinary meaning of interest by implying that it is the presence of an object that creates (or perhaps reorients) a desire which the object then uniquely satisfies. That object is the mediator.

Actor-network theory was built on case studies of the success—and especially the failure—of translation in this sense. Significant in the French science policy context were three failures: that of the electric car to be made publicly available (Callon and Latour 1981), the Minitel to become integrated into global computer networks (Castells 1996-98, 1:342-45), and a computer-driven customized rail system to appeal to Parisian commuters (Latour 1996). In each case, the failure

was traceable to an exaggerated confidence in what top-down management could accomplish without attending to the “interests” of those whose cooperation would be required for the policy’s implementation. It gradually became clear that these mediators held the balance of power. This point was underscored by the increased unemployment of all workers in the 1980s, with the exception of those who repaired and maintained the information technology that helped enable France’s rapid transformation from an industrial to a postindustrial economy (Gildea 1996, 102-3).

These facts, combined with the powerful role played by minor parties in French elections, undermined the myth that had been regularly deployed by both Bonapartists and Republicans for nearly two centuries to justify any number of French policy swings. According to this myth, France is a unitary nation run from the top by scientifically informed civil servants, quite literally “civil engineers.” Political parties provide alternative holistic visions of the best strategy for realizing France’s national destiny, which is typically pursued by a combination of world-historic diplomatic initiatives and protectionist economic policies. However, the combination of de Gaulle’s departure from public life in 1968 and the decline of Stalinist and Maoist factions soon thereafter placed these holistic visions in disrepute. It quickly became clear that opposing parties of the Right and Left could legitimate what turned out to be in practice largely the same policies. While public awareness of such ideological transparency has rendered the French electorate cynical of all professional politicians, at the same time it has instilled a spirit of compromise in those same politicians that had been previously lacking, which in turn has enabled coalition governments to stay in power longer than ever in the postwar period (Gildea 1996, 189-90).

This encapsulation of French politics displays a form of systemic rationality that would have met with Vilfredo Pareto’s approval: a circulation of elites who are sufficiently self-conscious that neither they nor their constituencies take their totalizing visions too seriously, given the political bottom line of “power” (in the case of the rulers) and “order” (in the case of the ruled). Under the circumstances, it is no surprise that the man most closely associated with “the end of ideology” thesis, Daniel Bell, has been the most influential American social theorist in contemporary France, the ultimate source of French theories of postmodernism, postindustrialism, and technocracy. In this postideological world, there is a new set of “angels” and “demons.”

The angels are intermediaries who, by adding or subtracting their support, keep the elites in constant circulation, thereby reinforcing the appearance of justice in the system (i.e., that every dog eventually has its day). The angels maintain equilibrium by preventing any party from fully dominating the system: in short, a power that only the weak can exert in a dynamic field of play. The demons are those who declare the entire system corrupt and propose in its place a radically new order that will end the need for all this political restlessness. In the French context, these demons have been clearly marked: the Right posed Jean-Marie Le Pen's National Front Party and the Left posed the Greens. Actor-network theory was conceived from the standpoint of the angels, which is to say, it provides a metaphysical justification for the political system remaining Paretian. Its critical edge lies solely in reminding policy makers not to get carried away by their own rhetoric. Thus, the stress on networks that extend unexpectedly across national boundaries can be seen as an antidote to hyperbolic claims for central planning that harken back to France's founding unitarian myth.

In that respect, the power that Callon and Latour themselves exert as social analysts is a by-product of their having tempered the claims of seemingly more powerful actors. Their work, in turn, has helped establish a rhythm to the circulation of elites that is tolerable by the society as a whole. This message has had a special resonance in France, given the status degradation of knowledge workers ever since de Gaulle damned academics and professionals by giving them exactly what they had asked for, namely, democratized entry into their ranks. So implemented, this legacy of the "Spirit of '68" simply confirmed the Machiavellian maxim that the multiplication of allies is the best way to divide loyalties and dissipate power. Of special relevance to L'École des Mines was that once their ranks swelled, engineers found it more difficult than ever to act as a united front in political negotiations. The divergent educational backgrounds and career trajectories of engineers seemed to render the idea of professionalism obsolete. Some have turned to union-like behavior, while others, fearing their chances of promotion, have sought forms of organization that do not threaten management's ultimate control of the means of production (Krause 1996, 158ff.).

de Gaulle's original policy of democratization—some would say proletarianization—has intensified with each successive French government, regardless of party. Moreover, it has taken several forms. At

the most basic level, university enrollment grew threefold from 1960 to 1970, and then doubled again by the end of the 1980s. The number of universities and research clusters, although still concentrated in metropolitan Paris, has increased, thereby reducing the public image of academic leaders from trusted mandarins to feuding warlords. (In the field of STS alone, there are at least six research units in the Paris area.) But most significantly, *les grandes écoles* have been forced to change their mode of domination in French society. While the admissions policies of these institutions are no less elite than in the past, the power their graduates exert in French society has generally declined, and the locus of power has shifted from the humanities and public administration schools to those like L'École des Mines, devoted to applied science, technology, and business (Bourdieu [1989] 1996).

To be sure, Mitterand had failed in the 1980s to democratize entry into *les grandes écoles*. Nevertheless, the value of professional degrees in engineering declined with the expansion of less prestigious engineering schools. This has had profound implications for STS, and sociology more generally, in France. First, it drove home a point of logic: it is possible for most top managers to be drawn from elite schools, while the likelihood of an elite graduate becoming a top manager diminishes. This is possible in a society where the elite can control *only* entry into its own immediate ranks but not the constitution of the field of play, which may include additional competitors who bring other qualities that alter the criteria of, say, leadership potential. Perhaps the most important intellectual legacy of this shift has been Pierre Bourdieu's ([1979] 1984) concept of "symbolic capital" as a form of knowledge-based power that is only partially determined by "cultural capital" (e.g., the quality of one's upbringing and training) yet potentially convergent with "economic capital" (e.g., where competition for top posts approximates a free market).

Considering Bourdieu's largely antagonistic stance to the actor-network approach, it is ironic that his conceptual innovation was introduced to the anglophone world by Latour and Woolgar (1979, esp. chap. 5). Bourdieu and Latour can be seen as trying to capture the same transformation from opposing perspectives: Bourdieu, the director of the leading state-supported research institute in the social sciences, critiquing the ways the state has buckled under external economic pressures; Latour, the resident sociologist at a leading beneficiary of the emerging neoliberal order, denying that the state ever had much control in the first place.

The locally effective but globally indeterminate sociology of translation offers a realistic vision for engineers whose “civil” status has been challenged by this self-inflicted neoliberal state of nature that forces each person to look after his or her own interests. But even if engineers no longer draft the blueprints for the future governance of society, they nevertheless remain instrumental in whether or not particular emerging tendencies acquire “forward momentum,” in the words of fellow traveler Thomas Hughes (1987).

Many recent strands in philosophy and sociology have drawn attention to the role of mediators who, though weak in themselves, are able to block or enable major alignments of knowledge and power (e.g., Rouse 1996, chap. 7). The distinctive feature of Callon and Latour, which has undoubtedly contributed to their appeal among STS researchers, is their explicit claim that the analyst herself accrues power in this mediating function. In contrast to Max Weber’s image of the sociologist who proposes feasible means to ends that have been selected by the policy maker, Callon and Latour (1981, 300-301) propose to show just how little real power the policy maker exerts and thereby demonstrate their own indispensability to the policy process.

The idea that an initially weaker party may directly benefit by displaying the weakness of others is the mark of what Latour’s philosophical mentor, Michel Serres (1980), has called the *parasite*, a concept simultaneously modeled on the presence of noise in a communication channel and the generosity of hosts who indulge an unwanted guest. The STS researcher thus becomes the ultimate noisy guest. The general strategy behind this move is fairly straightforward: Weber has been turned on his head. If the “modernity” of the state is marked by its reliance on scientifically authorized modes of legitimation, then instead of indulging their masters in the belief that policy regimes can be rendered efficient, duly authorized social scientists can both prove their usefulness and run interference on state policy by highlighting unforeseen obstacles on the way to policy implementation. The sociologist is thus able to manufacture a sense of integrity and even value-neutrality—along with a hint of radicalism—in a client-driven world: she can stare down her master while reinforcing the master’s need for her services. A not inappropriate comparison may be the psychotherapist who strings along the patient for the material benefit of the former and the spiritual benefit of the latter.

Beyond this rather strategic approach to research promoted by the Paris School, there has been, of course, a more generalized cynicism

toward the university, in the wake of de Gaulle's institutional reforms. Most striking in this regard is the provenance of the "post-modern condition" in which many intellectuals think we live. The famous report by that name that Jean-Francois Lyotard wrote for the higher education council of Quebec in 1979 is dedicated to the department where he held a chair in one of the new universities of Paris, wishing that it may flourish while the university itself withered away (Lyotard 1983, xxv; for a critique, see Fuller 1999b).

From Lyotard's Parisian perch, de Gaulle's attempt to placate the academic radicals who demanded more open admissions to elite institutions only served to co-opt them and compromise the independent standing of academia in French society. It was the state's last desperate attempt at maintaining social order in a world that was quickly exceeding its control. In this context, the appeal to specifically academic standards of discourse—including its more philosophically embroidered forms, such as Habermas's "ideal speech situation"—appeared as a disguised reactionary ideology for arresting the cross-fertilization of ideas and the novel developments they breed, which had increasingly come from outside academy (Lyotard 1983, 65ff.). Thus, in Lyotard's hands, the "university" was reduced from a transcendental condition for the possibility of critical inquiry to a cluster of buildings where representatives of these discourses have chance encounters and set up temporary alliances, subject to the terms set down by buildings' custodians (a.k.a. academic administrators). Notwithstanding Latour's (1993) protests to the contrary, actor-network theory should be seen as extending the postmodern condition from the humanities to the science and engineering faculties.

#### THE POLITICAL RAMIFICATIONS OF THE PARIS SCHOOL MENTALITY

The preceding social epistemology of the Paris School of STS should remind the reader of an elementary lesson in the sociology of knowledge, namely, that *a seemingly radical innovation that quickly acquires widespread currency probably serves some well-established interests that remain hidden in the context of reception*. This much Marxism understood, which is why the mobilization of class consciousness—consolidating the powerless into a source of power—would have to precede any genuinely progressive revolution. The failure of Marxists



to follow through on this strategy is now commonly seen to imply that the strategy itself was in error. This results in a political complacency that explains the curious fate common to Kuhnian paradigms and Latourian actor-networks, whereby surface ruptures of the status quo are accompanied by the containment of political possibilities. A good case of this “containment in action” is a well-regarded piece of STS normal science, Donald MacKenzie’s (1995) *Knowing Machines*, which applies a version of social constructivism influenced by actor-network theory to the careers of various forms of high technology, ranging from jet airplanes to mainframe computers.

MacKenzie, probably the most academically prominent student of the Edinburgh School, is a self-styled socialist who is not afraid to debunk determinist theories of technology associated with orthodox Marxism. This is a fair task for social constructivism. It should eventually lead to an enlargement of the sphere of public action. In the spirit of constructivism, MacKenzie argues that when the exact identity of a technological innovation has yet to be consolidated, competing perceptions of the technological horizon delimit the field of possibilities for what counts as technological change. The alternative lines of development can be shown to empower different constituencies. However, MacKenzie’s own case studies leave the impression that once the identity of a technology becomes relatively stable, the only way to induce further change is by capitalizing on what he calls “insider uncertainty,” namely, internal disagreement among the technology’s recognized experts. In actor-network jargon, the only people qualified to open a “black box” are those who have been in a position to close it.

This is a remarkably elitist image of the prospects for technological change, one that owes more to Pareto or Joseph Schumpeter than Marx (MacKenzie 1995, 16-17). Taken to its logical conclusion, it produces the following critique of feminist deconstructions of technology: “If Foucault is right that truth and power are intimately intertwined, then those seeking to change the world might try strategies to recruit powerful allies rather than assuming that the quest for revealing the truth will, in and of itself, lead to dramatic changes in levels and forms of inequality” (Grint and Woolgar 1995, 306). While there is certainly nothing wrong with exhorting constructivists and feminists to do more than simply publish their critiques in academic journals, it is telling that the only avenue recommended for getting action on those critiques involves courting the actually powerful few, as opposed to



organizing the potentially powerful many. Thus, we encounter the limits of the elitist political imagination, which takes *what has been* to be the totality of *what can be*.

Another indicator is the tendency to repackage opportunism as audacity. The same article faults feminists and constructivists for "timidity" when they refuse to adapt their critiques to changing circumstances and audience (Grint and Woolgar 1995, 305). No doubt feminists and constructivists would regard the authors as advising a "sell out." At the very least, the authors' flexibility must be seen in the context of the ends justifying the means, perhaps the main Machiavellian motif. However, one virtue of the authors' dogged pursuit of the Machiavellian argument is that it forces would-be social critics, reformers, and radicals to respond by focusing on the *ultimate ends* of their activities. The original neo-Machiavellians, Pareto and Schumpeter, were quite clear that the coalition of elites needed to maintain power is always unstable and, hence, subject to cycles of subversion, or "circulation." But they were equally clear that the populace at most provides the official pretext for change (e.g., an election, a policy initiative taken "on behalf of the people") and more often function simply as pawns, or "deployable resources," in Latourspeak.

This last point is worth stressing because actor-network theory is full of emancipatory-sounding talk that claims to reveal the "missing masses" needed for any large-scale sociotechnical achievement. However, the masses are presented as if they were literally physical masses whose movement is necessary to give an elite forward momentum. The agency of these masses is thus limited to the extension or withdrawal of collaboration, not the initiation of action. The current fashion for distributing agency across both people and things merely underscores the value of the masses as means to the ends of other parties, since in many cases nonhumans turn out to be at least as helpful as humans in achieving those ends. (The locus classicus is Callon 1986; for subsequent applications, see Ashmore and Harding 1994.) Although actor-network enthusiasts often make much of the innovative political vision implied in this extension of agency from persons to things, some disturbingly obvious precedents for this practice seem to have been suppressed from STS's collective memory, the first from *capitalism* and the second from *totalitarianism*.

The first precedent concerns actor-network theory's affinity with the metaphysics of *capitalism*, which, through the process of commodification, enables the exchange of human and machine labor on the basis of such systemic values as productivity and efficiency. This is the

sense in which technology is normally regarded as a “factor of production,” that is, a potentially efficient replacement of people. Indeed, the metaphysically distinctive tenet of *socialism* in modern political economy has been its revival of the medieval doctrine that human beings are the ultimate source of value in the world. But like capitalist cost accounting, actor-network theory knows no ontological difference between humans and machines. Consequently, the subtext of the title of Latour (1993), *We Have Never Been Modern*, might have read “We Have Never Been Socialist” to capture the increasingly neo-liberal climate of French science policy that makes ontological leveling seem so attractive. This point is lightly veiled in Latour’s refashioning of the word “delegation” to capture the process whereby humans and nonhumans exchange properties, which legitimates the treatment of humans as cogs in the wheels of a machine, and machines as natural producers of value.

Here we might compare the Parisian treatment with the most developed set of arguments for extending agency to nonhumans. These fall under the rubric of “Animal Liberation,” as popularized by the Australian moral philosopher Peter Singer (1975). In this guise, the politics of agency veers toward restraint and caution rather than mobilization and facilitation. An important difference between Singer and Latour is that the Animal Liberation movement has gravitated toward a conception of “animal rights” modeled on the civil rights accorded to humans. Significantly, a sentient creature, usually a mammal, is the paradigm case of a “nonhuman.” In contrast, the various Parisian exemplars of a “nonhuman” have typically resided much lower on the evolutionary scale: scallops, microbes, and even mechanical door closers all serving as examples at various points (Callon 1986; Latour 1988, 1995). The overall effect is that in its proliferation of agency, actor-network theory dehumanizes humans, while Animal Liberation humanizes animals.

When Hegel, following Spinoza, said that freedom fully realized is the recognition of necessity, he had in mind an idea that can easily be lost in the liberatory rhetoric associated with the extension of agency to nonhumans, namely, that to increase the number of agents is not to increase the amount of agency in the world. On the contrary, it is to limit or redefine the agency of the already existing agents. A’s full recognition of B’s agency requires that A either make room for B as a separate agent or merge with B into a new corporate agent. In both cases, A is forced to alter its own identity. In the former case, the change may be rationalized as A’s coming to lead a simpler life,

whereas in the latter, it may be rationalized as A's now having access to more power than before. The former corresponds to Animal Liberation, the latter to actor-network theory: the former retains the human as unique agent (at least at the species level) but at the cost of diminished wants and power, whereas the latter magnifies the wants and power of the human but at the cost of rendering each individual a (potentially replaceable) part of the larger corporate machinery. (For an earlier treatment that mistakenly assimilated actor-network theory to the Animal Liberationist perspective, see Fuller 1996.)

Animal Liberation's excesses are regularly documented in the forced entries into university laboratories to "free" animals that have been caged for experimental purposes. Yet, there is an even less savory precedent for the extremes to which an actor-network perspective can be taken, namely, the twentieth century's unique contribution to political theory and practice: *totalitarianism*. Contrary to Latour's oft-repeated claim that politics has never taken technology seriously, totalitarian regimes stand out from traditional forms of authoritarianism precisely by the role assigned to technology as the medium through which citizens are turned into docile subjects, specifically, parts of a corporate whole.

While attention has usually focused on totalitarian investments in military technology, of more lasting import have been totalitarian initiatives in the more day-to-day technologies associated with communication, transportation, and building construction. The early stages of these developments already informed science policy debate in continental Europe at the dawn of the twentieth century (Fuller 2000, chap. 2, sec. 3). Ultimately, these technologies enabled unprecedented levels of mass surveillance and mobilization, all in the name of configuring the national superagent. In the course of this configuration, any sharp division between humans and nonhumans was removed. An important consequence was that a subset of the human population—say, the Jewish race or Communist ideologues—could be excluded from the corporate whole as such great security risks that the rest of the human population would agree to submit themselves to sophisticated invasive technologies in order to become part of, say, the "Nazi cyborg."

This last point was first made by Carl Schmitt, the Weimar jurist who provided the original legal justification for the one-party state that became Nazi Germany. Schmitt ([1932] 1996) held that technology was the latest and most durable corporate glue because its apparently neutral character seemed to impact everyone equally, thereby

enabling conflict to metamorphose from the elite cross-border confrontations of the past to “total war” involving a nation’s entire population. Schmitt envisaged that the threat of an external foe more powerful than any internal foe would lead citizens to submit to the application of mass technologies for purposes of defeating that foe, however much their own personal freedoms may be constrained.

Actor-network theory can be understood as the account of society that results once there is no longer a hegemonic state apparatus in charge of this technostucture: a devolved totalitarian regime; in a phrase, *flexible fascism*. Instead of a unitary state that renders everyone a means to its specific ends, now everyone tries to render everyone else a means to their own ends. The former members of the corporatist state may have lost their sense of common purpose, but they retain the personal ethic which attended that purpose. The difference in actual outcomes is much less predictable than under a totalitarian regime, but ultimately explainable in terms of the agents’ differential access to the resources needed to attain their ends. Thus, the necessitarian myths that originally propped up Mussolini, Hitler, and Stalin have now yielded to contingent narratives centered on Pasteur (by Latour), Edison (by Hughes), and Seymour Cray (the inventor of the main-frame computer, by MacKenzie).

Indeed, one of the eerier similarities between the predilections of totalitarian and actor-network theorists is the glorification of the heroic practitioner—be it the power politician or the heterogeneous engineer—whose force of will overcomes the self-imposed limitations of superstitious citizens and academics in the grip of a theory. Thus, comparable to Pareto’s disdain for the planning pretensions of social democrats is Callon’s (1987, esp. 98ff.) contempt for the sociologists Pierre Bourdieu and Alain Touraine, who define in mere words the contemporary state of French society, something engineers supposedly do much more effectively in their daily practice.

One of the most remarked upon features of fascist ideology is its easy combination of an animistic view of nature, a hyperbolic vision of the power of technology, and diminished sense of individual human agency. The same could be said of the “delegations” and “translations” that characterize the accounts of sociotechnical systems provided by actor-network theory. Interestingly, in his brief discussion of totalitarianism, Latour (1993, 125-27) comes closest to endorsing the Pirandellist “it is so, if you think so” form of relativism of which his critics have often accused him. Specifically, he explains the formidability of totalitarian regimes in terms of a widespread

belief in their underlying philosophies, rather than, say, the collective impact of the actions taken under their name. Latour officially wants to ensure that people are not inhibited by philosophies that stray too far from the scene of action, but his argument also implies that one ought not be inhibited from forming alliances with people to whom such philosophical labels as “totalitarian,” “capitalist,” and “imperialist” are conventionally attached. In this way, Latour allows nominalism all too easily to slide into opportunism.

However, actor-network theory’s resonance with totalitarianism loses its shock value, once we recall its origins in the training of an engineering profession that over the past quarter-century has lost its custodianship of a unitary French state. This lost world is the technocratic side of French thought that began with Napoleon’s establishment of the *polytechniques*, was apotheosized by Comtean positivism, and has now reached its decadent phase with actor-network theory. Its spirit moves imperceptibly through an anglophone STS community that still harbors the stereotype of “French intellectuals” as the consciences of their times—individuals with a spiritual paternity traceable to Emile Zola and Jean-Paul Sartre—even though 15 years has now passed since Michel Foucault’s death.

#### THE FRENCH PHILOSOPHICAL CONNECTION: BACHELARD AS OBLIGATORY PASSAGE POINT FOR STS

Readers who like their philosophy served in pure form may be inclined to dismiss the preceding explanation of actor-network theory’s ascendancy for being too much based on the peculiarities of the French scene. But not to worry: there is a more properly philosophical account, one that takes its cue from that godfather of French structuralism and poststructuralism, Gaston Bachelard (Lecourt 1975 is still the most relevant introduction).

Bachelard had a peculiar way of envisaging the relationship between scientists and philosophers that was compelling to a French intellectual culture that has never gotten over its initial love affair with Marx. According to Bachelard, scientists most closely approximate the ideal of *homo faber*, humanity in a state of world making, whose work is organically integrated into the world: we are all constructivists before we are anything else. From the academic Marxist perspective that gave Bachelard his widest reception in France of the

late 1960s and early 1970s, he seemed to portray scientists as a proletariat exploited by bourgeois philosophers who held them accountable to standards not of their own making. At the institutional level, Bachelard's defense of scientists from philosophers should be read as a moment in the resistance of the polytechniques from the domination of the universities. He was trying, in the French context, to reverse the tendencies that were represented in 19th-century Britain by William Whewell, who sought to portray technical innovations as "always already" theorized (Fuller 2000, chap. 1, secs. 6-8).

For Bachelard, philosophers derive the conceptual equivalent of surplus value from performance standards that transcend the scientists' own horizons. These standards typically adopt a frame of reference that refers either to the beginning or the end of inquiry, that is, when science is still an idea in the scientist's mind or in fully finished form as an artifact or experimentally reproducible effect: roughly speaking, "idealism" (subjectivism) versus "realism" (objectivism). From either temporal end of the process, actual scientific work appears to fall short of the presumed philosophical norm. This provides the *raison d'être* for philosophical intervention: scientists are berated whenever they fail to meet the norm, while philosophers claim credit whenever scientists conform to it. Thus, Mach's failure to accept the existence of atoms and discover relativity theory is traceable to his opposition to scientific realism, whereas Einstein's success on both counts is attributable to his commitment to said philosophy. In this respect, the philosopher is part ventriloquist, part alchemist. Metaphysical ideas are projected into scientific work, only to be retrieved as the "essence" of that work, with the residual aspects then treated as in need of philosophical mediation. In this way, any scientific achievement can be reduced to its rational essence and various arational deviations. To be sure, this strategy is not limited to the francophone philosophical world, as the ongoing anglophone debate between "realists" and "relativists" demonstrates.

That the ultimate meaning of science comes from philosophy is persuasive just as long as philosophy speaks in one voice, and hence science is shown to be single-minded in its aims. However, Bachelard's point was that philosophy pulls science in many contradictory directions, more in the manner of ideology than an account of reality supposedly superior to science. In his characteristically analytic "philosophical topology," Bachelard—and Latour after him—depict this phenomenon in terms of gradients of "displacement" or "disper-

sion" from any number of presumed philosophical norms (cf. Lecourt 1975, 44ff.; Latour 1993, chap. 3). What centers these displacements and dispersions is the actual work of science. By so privileging science as the "immutable mobile" of philosophical inquiry, Bachelard had overturned the *entente cordiale* between philosophers and sociologists, which involved treating natural science as an object teleologically drawn toward the philosophical domain of reason, absent the interference of sociologically defined forces. In effect, this Aristotelian image was rendered Newtonian, as natural science was shown to be driven by its own inertial impulse, subject to the competing pulls of various philosophies acting at a distance.

In the wake of Bachelard, STS has not only privileged science over philosophy, but has uncannily reproduced the value orientation of contemporary Big Science in the sites it has chosen for study. (An especially clear and recent example is Knorr-Cetina 1999, a comparative ethnography of the European particle accelerator CERN and a major molecular biology laboratory.) Moreover, the field has continued the misleading impression that one is a scientist only in research, but not in teaching or administration. Of course, STS typically tells a rather different story from the one scientists or philosophers are inclined to tell about these privileged research sites. More people and things are incorporated into the STS narratives, which complicates the picture of how science manages to do as well as it does. But at the same time, the added complication diffuses responsibility for any of the actions taken in the name of science. On the one hand, this helps redistribute the credit for scientific work from the few "geniuses" who normally receive all the glory; on the other hand, it makes it difficult to hold anyone accountable for anything.

Consider the actor-network vision of technoscience emanating from Paris. What, on a sympathetic reading, may appear to be an amorphous network of highly contingent nodes may be portrayed, less sympathetically, as an all-pervasive system whose general structure cannot be purposefully altered by some strategic intervention, let alone a social movement. In this way, STS practitioners may be able to continue their steady stream of detailed case studies for both collegial and cliental consumption without offering counsel to those interested in a fundamental renegotiation of science's social contract. It may be, then, that the joke is on us when we fail to recognize that Latour was playing it straight when he told an interviewer that STS does not pose any serious threat to the scientific establishment (Crawford 1993).



### CONCLUSION: TOWARD FILLING THE NORMATIVE VACUUM OF STS

The interpenetration of science and society so vaunted by STS rhetorically functions to discourage inquirers from looking far beyond those objects of fascination—the laboratories—to see how science reflects larger societal forces. Instead, science is portrayed as “always already social,” which implies that whatever larger forces need to be taken into account will be “inscribed” in the people and things located in the laboratory. Not surprisingly, STS practitioners have endured an uneasy relationship with Marxist and feminist science critics. For example, in the ongoing “science wars,” they have been united more in terms of a common foe—the scientific establishment—than a common methodological and axiological orientation.

More generally, STS conveys a surface radicalism, in that the analyst is supposed to suspend any technical knowledge that she might have of the practices under study. The subversive epistemological consequence is that the STS practitioner often notices things, especially incongruities between word and deed, that escape the practicing scientist’s attention. But the subversion only goes so far, as STS’s own critical potential is truncated by the symmetrical tendency to suspend any technical knowledge of the *social sciences* that STS practitioners might bring to bear on the case. While such methodological asceticism (a.k.a. “grounded theory”) is not without precedent in the sociological literature (e.g., Glaser and Strauss 1967), it has had the effect of precluding political factors that do not explicitly enter the scientists’ own deliberations.

For example, STS practitioners employ discourse-analytic techniques to reveal the various voices in a language game played by a community of scientists, but they do not use the techniques to engage in an ideology critique of science that appeals to factors that sustain the game but transcend the scientists’ control or awareness. It is as if a postmodernist aversion to proffering master narratives has forced the STS researcher into the role of simply stripping away narratives that “others” have imposed on the subject under study, in the hope that something meaningful will remain to be said. (For an explicit defense of this point, see Latour 1993, esp. 5-8, 122-27.) In the event that this does not happen, one can simply revel in the “chaotic” or “rhizomatic” character of the subject’s behavior.

From the standpoint of social epistemology (Fuller 1988, 1993), STS suffers from normative confusion, an incapacity to pronounce on



whether it likes or dislikes what it so perspicuously sees. If “relativist” is the name that a philosopher or scientist gives to someone who raises an inconvenient fact against an incontrovertible truth, then “universalist” is the name that a historian or sociologist gives to someone who insists on deciding whether she would want to live in the world that she describes.

At the risk of being accused of universalism, then, I would like to query the locus of normativity in the scientific enterprise. Many recent social histories of science stress the role played by technicians and other on-site laboratory personnel in the production and maintenance of apparatus needed to persuade onlookers that an experiment has worked properly. These people generally went unrecognized in their day. What is the normative conclusion that should be drawn from this? Is it something akin to a labor theory of value—those who do the work deserve the credit? Yet, as suggested above, the more populated the world of technoscience, the more diffuse the assignment of credit and blame—that is, unless some explicit attempt is made to remove credit or blame from others who have held it before.

Moreover, even this incipient labor theory of value omits the role that those away from the site of original knowledge production—the colleagues, policy makers, teachers, students—play in conferring a “scientific” status on the fruits of the site’s labors. Given the constructivist methodological maxim of defining scientific practices by their consequences rather than their causes, it would seem that regardless of the effort spent in the laboratory—be it by big-name scientists or no-name technicians—the final determinant of scientific status is the community of recipients, which may well be more democratically distributed than the community of producers. In that case, should not STS practitioners ultimately prefer a theory of scientific value based on *utility* rather than labor? Unfortunately, this question remains both unanswered and, more tellingly, *unasked* in the STS literature (most noticeably in Latour 1987).

Of course, things need not be that way, since the possible ways forward are fairly obvious. Just focusing on the relationship between scientists’ words and deeds, at least three gambits are available to the STS community (Fuller 1992):

1. Openly admit that science is no more or less truthful, rational, objective, and so on than other social practices, and conclude that either the status of science needs to be lowered or the status of other social practices needs to be raised.

2. Argue that the special talk surrounding science may have no binding force on the actions performed at the research site but does constrain the possibilities for action in administrative and educational settings, where the appeal to science serves a more explicitly legitimatory function. In that case, those interested in witnessing the distinctive power of science would do better focusing their attention on these distribution points rather than the original "hands-on" sites of production.
3. Somehow try to regiment scientific discourse to live up to its own normative ideals by subjecting scientific claims to greater scrutiny than one would ordinarily claim. This would entail a level of suspicion and discipline that would effectively undermine the so-called tacit dimension that has traditionally conferred on scientific knowledge its status as expertise.

If these courses of action are so "obvious," why are they not currently pursued by STS researchers? An important part of the answer lies in the aversion or inability of STS researchers to adopt a perspective independent of either those under study or, more saliently, those for whom the study is done. Latour (1997) illustrates the lengths that some STS researchers will go to pursue the autonomy of scientific practice from science critique. He argues that critique is morally objectionable because it presupposes a low opinion of scientific practitioners, whose alleged self-deception provides the only opportunity for the critic to practice her own trade. Critics treat practices as mere means to their own ends, while failing to recognize that the most perfect constructions are ones whose handiwork is hidden and, hence, without need of critical improvement.

Latour's etiology of the critic's craft is based largely on Bachelard's defense of scientific labor from philosophical exploitation, as discussed in the previous section. However, Latour's equation of normal scientific practice with seamless construction harkens to a version of the theological argument from design, namely, the postulation of a *deus absconditus*—a God who builds the world so well that his services are no longer required. Thus, science works so well that its constructed character does not matter, and philosophers are invited to take leave. The STS researcher, then, is like the Wittgensteinian who refuses to revise, let alone improve, our understanding of the world, but merely holds a mirror to it. In this respect, the epistemological chicken debate discussed in the first section of this article does not go very deep at all: both Collins and Latour are beholden to the late Wittgenstein's normative quietism.

I wish to recover what lies outside the Wittgensteinian horizons of what Latour (1993, 1997) himself positively dubs "acritical" STS. This

is the tricky proposition to which my project of social epistemology has been devoted for over a dozen years. It involves defining the ends of inquiry in relative independence from whatever outcomes most benefit the population of researchers or their clients at a given place and time. It requires, among other things, frank recognition that the ends of those we study may be antagonistic to those who sustain our livelihood. I believe that this will be “essential tension” that defines the normative horizon of STS in the coming years. In a new book on “the governance of science” (Fuller 1999a, esp. chap. 1), I characterize this problem in terms of “financial censorship,” whereby the costs of sustaining systematic research have become so high that clients and benefactors can effectively discourage lines of inquiry that threaten to upset their interests. Overcoming this problem—which may require a Feyerabendian approach to downsizing research budgets—calls for a new injection of the kind of insight that traditionally philosophers have been capable of giving. The benefit will be a renewed sense of intellectual integrity that is currently lacking in this most avant-garde of academic pursuits.

#### REFERENCES

- Ashmore, Malcolm, and Stella Harding, eds. 1994. Humans and others: The concept of agency and its attribution [Special issue]. *American Behavioral Scientist* 37:731-856.
- Becker, Howard. 1967. Whose side are we on? *Social Problems* 14:239-47.
- Bourdieu, Pierre. [1979] 1984. *Distinction*. Reprint, Cambridge, MA: Harvard University Press.
- . [1989] 1996. *The state nobility*. Reprint, Cambridge: Polity Press.
- Callon, Michel. 1986. Some elements of a sociology of translation: Domestication of the scallops and the fishermen. In *Power, action, and belief*, edited by J. Law, 196-229. London: Routledge.
- . 1987. Society in the making. In *The social construction of technological systems*, edited by W. Bijker, T. Hughes, and T. Pinch, 83-106. Cambridge, MA: MIT Press.
- Callon, Michel, and Bruno Latour. 1981. Unscrewing the Big Leviathan. In *Advances in social theory and methodology*, edited by K. Knorr-Cetina and A. Cicourel, 277-303. London: Routledge.
- Callon, Michel, John Law, and Arie Rip. 1986. *Mapping the dynamics of science and technology*. London: Macmillan.
- Castells, Manuel. 1996-98. *The information age*. 3 vols. Oxford: Blackwell.
- Collins, Harry, and Steven Yearley. 1992. Epistemological chicken. In *Science as practice and culture*, edited by A. Pickering, 301-27. Chicago: University of Chicago Press.
- Crawford, Hugh. 1993. An interview with Bruno Latour. *Configurations* 2:247-69.
- Fuller, Steve. 1988. *Social epistemology*. Bloomington: Indiana University Press.

- . 1992. Social epistemology and the research agenda of science studies. In *Science as practice and culture*, edited by A. Pickering, 390-428. Chicago: University of Chicago Press.
- . 1993. *Philosophy, rhetoric and the end of knowledge*. Madison: University of Wisconsin Press.
- . 1996. Talking metaphysical turkey about epistemological chicken. In *The disunity of science*, edited by P. Galison and D. Stump, 170-88, 468-71. Palo Alto, CA: Stanford University Press.
- . 1999a. *The governance of science: Ideology and the future of the open society*. Milton Keynes: Open University Press.
- . 1999b. Making the university fit for critical intellectuals: Recovering from the ravages of the postmodern condition. *British Education Research Journal* 25:583-96.
- . 1999c. Review of Castells (1996-8). *Science, Technology & Human Values* 24:159-66.
- . 2000. *Thomas Kuhn: A philosophical history for our times*. Chicago: University of Chicago Press.
- Gibbons, Michael, C. Limoges, H. Nowotny, S. Schwartzman, P. Scott, and M. Trow. 1994. *The new production of knowledge*. London: Sage.
- Gildea, Robert. 1996. *France since 1945*. Oxford: Oxford University Press.
- Glaser, Barney, and Anselm Strauss. 1967. *The discovery of grounded theory: Strategies for qualitative research practice*. Chicago: Aldine.
- Golinski, Jan. 1998. *Making natural knowledge*. Cambridge: Cambridge University Press.
- Gouldner, Alvin. 1968. The sociologist as partisan: Sociology and the welfare state. *American Sociologist* 3:103-16.
- Grint, Keith, and Steve Woolgar. 1995. On some failures of nerve in constructivist and feminist analyses of technology. *Science, Technology & Human Values* 20:286-310.
- Hughes, Thomas. 1987. The evolution of large technological systems. In *The social construction of technological systems*, edited by W. Bijker, T. Hughes, and T. Pinch, 51-82. Cambridge, MA: MIT Press.
- Knorr-Cetina, Karin. 1999. *Epistemic cultures*. Cambridge, MA: Harvard University Press.
- Krause, Elliott. 1996. *Death of the guilds: Professions, states, and the advance of capitalism, 1930 to the present*. New Haven, CT: Yale University Press.
- Labinger, Jay. 1995. Out of the petri dish endlessly rocking. *Social Studies of Science* 25:341-48.
- Latour, Bruno. 1987. *Science in action*. Milton Keynes: Open University Press.
- . 1988. *The pasteurization of France*. Cambridge, MA: Harvard University Press.
- . 1993. *We have never been modern*. Cambridge, MA: Harvard University Press.
- . 1995. Mixing humans and non-humans together: The sociology of the door-closer. In *Ecologies of knowledge: Work and politics in science and technology*, edited by S. L. Star, 257-89. Albany: State University of New York Press.
- . 1996. *Aramis*. Cambridge, MA: Harvard University Press.
- . 1997. A few steps toward an anthropology of the iconoclastic gesture. *Science in Context* 10:63-83.
- Latour, Bruno, and Michel Callon. 1992. Don't throw the baby out with the Bath School! In *Science as practice and culture*, edited by A. Pickering, 343-68. Chicago: University of Chicago Press.
- Latour, Bruno, and Steve Woolgar. 1979. *Laboratory life: The social construction of scientific facts*. London: Sage.

- Lecourt, Dominique. 1975. *Marxism and epistemology: Bachelard, Canguilhem, Foucault*. London: Verso.
- Lyotard, Jean-Francois. [1979] 1983. *The postmodern condition*. Reprint, Minneapolis: University of Minnesota Press.
- MacKenzie, Donald. 1995. *Knowing machines*. Cambridge, MA: MIT Press.
- Rouse, Joseph. 1996. *Engaging science*. Ithaca, NY: Cornell University Press.
- Schmitt, Carl. [1932] 1996. *The concept of the political*. Reprint, Chicago: University of Chicago Press.
- Serres, Michel. 1980. *La parasite*. Paris: Grasset.
- Singer, Peter. 1975. *Animal liberation*. New York: Random House.
- Star, Susan Leigh. 1995. Introduction. In *Ecologies of knowledge: Work and politics in science and technology*, edited by S. L. Star. Albany: State University of New York Press.
- Turney, Jon, ed. 1984. *Sci-tech report: Everything you need to know about science and technology in the 80s*. New York: Pantheon.

*Steve Fuller is a professor of sociology at the University of Warwick. He founded the research program of social epistemology shortly after receiving a Ph.D. in history and philosophy of science at the University of Pittsburgh in 1985. Since then he has written many articles and several books, the most recent of which are Science (Open University Press, 1997), The Governance of Science: Ideology and the Future of the Open Society (Open University Press, 1999), and Thomas Kuhn: A Philosophical History for Our Times (University of Chicago Press, 2000).*